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Dr. Auwers, Gotha;
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Dr. Forster, Berlin; and
Lieutenant Safford, Chicago,

were balloted for and duly elected Associates of the Society.

The Semidiameter of the Moon according to M. Hansen's Tables of the Moon, compared with the results of the best Observations. By J. A. C. Oudemans.

(*Translated from a translation into French communicated by M. Hoek.*)

Though complaints are made of the uncertainty of the planetary diameters, as obtained from micrometric measure-

ments, yet the diameter of the Moon is no better known to us. In 1851, 53, 54, and 58, M. U. De Lange and I have observed many occultations of the stars, which I have used to calculate the longitude of Batavia. The observations indicate clearly that the semidiameter of the Moon, as given by the Tables of M. Hansen, has need of a negative correction. To control this indication, I have brought together all the determinations known to me of the aforesaid element by means of total and annular eclipses, of occultations of stars, and of measurements made by the aid of a heliometer. I remark, first of all, that M. Hansen has adopted the arithmetical mean which the meridian instruments at Greenwich have given for the two diameters of the Moon, horizontal and vertical. Then, whatever errors there were in the expressions according to which Burckhardt has calculated his Tables of Lunar Parallax, these errors have an influence on the semidiameters deduced from observations at different periods. The most part of the calculations that I have employed give the correction of the semidiameter of Burckhardt's Tables. It was necessary then to reduce them to M. Hansen's Tables. I have done this by means of Mr. Adams' Tables (*Nautical Almanac*, 1856). The following are the details of my results :—

A. Occultations of the stars by the Moon.

1. Occultations of the *Pleiades*, 29th Aug. 1820, calculated by Rosenberger (*Königsberger Beobachtungen* IX *Abtheilung*, p. v.).

As the observation of this phenomenon had succeeded very well at Königsberg, and the positions of the stars were exactly known, Rosenberger has deduced from it the correction of the semidiameter of the Moon. Four immersions combined with six emersions give him

Correction of Burckhardt's Semidiameter	$= + 0''.07$

I find :—

Reduction of Burckhardt's Tables to those of Hansen ..	$= - 0''.93$
Correction of Hansen's Semidiameter	$= - 0''.86$

2. Occultation of θ' *Tauri*, 28th March, 1830, which M. Kaiser has used for the determination of the longitude of the Observatory of Leyden (*Memoirs of the R. A. S.*, vol. x. p. 303).

The immersions as well as the emersions were observed at Leyden, Dorpat, and Mannheim. The final equation—

$$\text{Corr. of the Semidiameter} = 2''.94 - 0''.0023\,\delta(\beta - b) + 0''.0004\,\delta\pi,$$

is perfectly applicable to our purpose. It will be sufficient to consider as zero the two quantities $\delta(\beta - b)$ and $\delta\pi$, which at most are not more than some seconds : whence—

Correction of Burckhardt's Semidiameter ..	+ 2″·94

But we have :

Reduction of Burckhardt's Tables to those of Hansen	− 3″·43
Correction of Hansen's Semidiameter	− 0″·49

3. Occultation of α *Tauri*, 10th February, 1832, also employed by M. Kaiser.

The complete observation was successful at Mannheim, Cambridge, Aberdeen, and Greenwich. The equations give :

$$\text{Correction of the Semidiameter} = + 0''{\cdot}33 - 0{\cdot}055\,\delta\pi.$$

I find :

$\delta\pi$ = Parallax according to Adams − that according to Burckhardt	= − 0″·46

Then :

Correction of Burckhardt's Semidiameter	= + 0″·36
Reduction to Hansen's Tables	− 1″·47
Correction of Hansen's Semidiameter	− 1″·11

4. Occultation of the *Pleiades* 10th Aug., 1841, calculated by Lejeune (*Dissertatio Astronomiæ Inauguralis*, Lugd. Batav., 1845).

An excellent determination resting on a great number of observations. Fifty-two equations combined according to the method of least squares, gave : $\delta l = + 1''{\cdot}33 - 0{\cdot}259\,\delta\pi$ with a probable error $\pm$ 0″·09 ; here $\delta l = \pi\,\delta\varkappa$, where π is the parallax, $\varkappa$ the ratio (0·2725) of the sine of the semidiameter to the sine of the equatorial horizontal parallax. The correction of the semidiameter is therefore—

$$\begin{aligned}\pi\,\delta\varkappa + \varkappa\,\delta\pi = \delta l \quad &+ 0{\cdot}2725\,\delta\pi \\ = 1''{\cdot}33 &+ 0{\cdot}0135\,\delta\pi.\end{aligned}$$

I find—

$\delta\pi$, or the parallax according to Adams − that according to Burckhardt	= + 3″·44

whence it follows,—

Correction of Burckhardt's Semidiameter	..	+ 1″·83
Reduction to Hansen's Tables ..	..	− 2·49
Correction of Hansen's Semidiameter ..	..	− 1·11

B. Total Eclipses of the Sun.

1. The Eclipse of July 7, 1842. M. Olufsen's calculations (*Astron. Nach.* xxii. p. 217) gave him,—

$$\pi \delta x = -2''{\cdot}05 - 0{\cdot}296\,\delta\pi$$

add to this—

$$x \delta\pi = \quad 0{\cdot}2725\,\delta\pi$$

and we shall have :

Correction of Burckhardt's Semidiameter $= -2''{\cdot}05 - 0{\cdot}0235\,\delta\pi$.

Mr. Adams' Tables (*Nautical Almanac*, 1856) give for this date $\delta\pi = -0''{\cdot}4$, therefore

Correction of Burckhardt's Semidiameter	..	= − 2″·04
Reduction to Hansen's Tables ..	..	= − 1·46
Correction of Hansen's Semidiameter ..	..	= − 3·50

The same eclipse has been calculated by Carlini. Unfortunately I could not at Batavia consult his Memoir, which is in the *Giornale dell' Instituto Lombardo*, vol. iv.

M. Santini has quoted the results in his account of the eclipse of July 28, 1851, these results differ very much from those of M. Olufsen : "Il semidiametro lunare," says Santini, "poi corrispondente alla parallasse equatoriale di 60′ fù dal Signor Carlini assegnato = 16′ 20″·4."

Burckhardt's Tables give for this date a parallax of 59′ 58″·8. The discussion of the observations by Carlini has therefore given a semidiameter during the eclipse of 16′ 20″ 07, while Burckhardt's Tables give 16′ 20″·7.

The result is :

Correction of Burckhardt's Semidiameter	..	− 0″·63
Reduction to Hansen's Tables ..	..	− 1·46
Correction of Hansen's Semidiameter ..	..	− 2·09

2. The total Eclipse of July 28, 1851, of which there are a great number of observations, but which have not yet been completely discussed. Santini submits the observations of ten observatories to calculation, and has deduced from thence the

corrections of latitude and longitude of the Lunar Tables (*Ast. Nach.* xxxiv. p. 289).

The observations at Dantzig have been employed there in the determination of the longitude of that place.

The four phases observed at Dantzig, when the erroneous quantity in the 12th equation + 5″·36 is replaced by − 5″·36, give :

Correction of the Semidiameter of the Sun	..	− 0″·65
,, ,,	Moon (Burckhardt)	− 0·75

while the observations at Königsberg where the eclipse was also total, gave to Santini :

Correction of the Semidiameter of the Sun	..	− 3″·07
,, ,,	Moon (Burckhardt)	− 0·70

The first of these two last quantities attains a value, improbable because of its magnitude, and incompatible with the results of the observations at Dantzig. The second, for this reason, deserves little confidence. But, fortunately, we have the researches of Wichmann, who introduces into his calculations the measurements of the distance of the horns (*Ast. Nach.* xxxiii. p. 309), and who obtains :

Correction of Burckhardt's Semidiameter of the Moon − 0″·79

which agrees well enough with the result of the Dantzig observations.

We have, then, for the date of this eclipse : —

Parallax according to Burckhardt (*Naut. Alm.*)	=	60′ 30″·2
Correction according to Adams (*Naut. Alm.*)	=	−0″·3
Parallax according to Hansen ..	=	60 29·9
Semidiameter corresponding to this Parallax	=	16 30·75
Semidiameter according to Burckhardt (Memoir by Santini)	=	16 29·2
Correction of Burckhardt's Semidiameter ..	=	−0·75
Reduction to Hansen's Tables ..		−1·55
Correction of Hansen's Semidiameter ..	=	−2·30

C. Annular Eclipses of the Sun.

1. The annular Eclipse of the 7th September, 1820, of which the observations have been calculated by several astronomers, Walbeck, Santini, Rümker, Wurm, and Bürg.

Walbeck has determined the moment of the conjunction according to the observations of seven observatories. Those of Göttingen, Cuxhaven, Bremen, and Mannheim, places where

the eclipse was annular, gave him, for the correction of the semidiameter, according to Burckhardt's Tables, the following quantities:—

The observations of	Gauss		+2".73
,,	,,	Harding	+3".79
,,	,,	Struve	+2".20
,,	,,	Walbeck	+2".02
,,	,,	Olbers	+1".06
,,	,,	Gildemeister	+0".85
,,	,,	Tralles	+0".10
,,	,,	Nicolai	+1".66
,,	,,	Heiligenstein	+1".21
		Mean	+1".74

I have combined the results of Walbeck's calculations in another manner.

The four astronomers whose names occur first in the preceding table, all observed at Göttingen; Olbers and Gildemeister at Bremen; Nicolai and Heiligenstein at Mannheim. Now it is clear that the local irregularities of the limb of the Moon have influenced alike all the observations in one place. I have, therefore, preferred the following combination:—

Mean of the observations at		Göttingen	+2".68
,,	,,	Bremen	+0".95
,,	,,	Cuxhaven	+0".10
,,	,,	Mannheim	+1".44
		Mean	+1".29

The semidiameter of the Moon, having been adopted by Walbeck = 881".02 (*Correspondance Astronomique*, iv. p. 501), we have, according to these calculations:—

Semidiameter of the Moon according to the observations of the Eclipse of the 7th September, 1820 .. .. 882".31

The calculations of Santini are mentioned at the same time as those of Walbeck in the report by Von Zach. "M. Santini," he says, "has also calculated a very great number of observations of this eclipse; he is working at a memoir, which he is going to publish shortly. In the meantime, these are some of the results which he has communicated to us:—

Correction of Burckhardt's Semidiameter of the Moon +1".39

I, therefore, admit here the same value of the semidiameter

as in Walbeck's calculations. On this supposition Santini's calculations lead to the result:—

Semidiameter of the Moon, 7th September, 1820 = 882″·41

Rümker's calculations (*Berliner Astronomisches Jahrbuch*, 1824, p. 153) embrace the observations made at 18 places. Among them are Amsterdam, Bergen, and Zurich, where the eclipse was annular, and also the four places already mentioned on the occasion of Walbeck's calculations. Rümker adopted—

Semidiameter of the Moon = 881″·0

and he found

Correction .. = +0·14

True Semidiameter .. = 881·14

Bürg's results rest on a total of 51 observations made in twenty-one places (*Berliner Astronomisches Jahrbuch*, 1824, p. 119). The elements of his calculations deduced from his own tables assume

Semidiameter of the Moon = 883″·1

a quantity for which the observations indicate

Correction of = − 2·3

from which it follows that

True Semidiameter = 880·8

Lastly, the *Berliner Astronomisches Jahrbuch* of 1825, p. 89, contains Wurm's calculations. A total of 172 observations made at 79 places were compiled by this calculator. Although he was obliged to reject many, particularly at the commencement of the eclipse, he succeeded in basing his results upon a number of observations far larger than that employed by the other calculators. He finds—

Correction of the Semidiameter adopted −2″·18

Semidiameter adopted in the calculations 882·90

True Semidiameter of Sept. 7th, 1820 = 880·72

By means of Mr. Adams' Tables I find—

The Semidiameter according to Hansen's Tables = 883″·35

from whence it follows: —

Correction according to	Walbeck	$= -1''.04$
,, ,,	Santini	$= -0''.94$
,, ,,	Rümker	$= -2''.21$
,, ,,	Bürg	$= -2''.55$
,, ,,	Wurm	$= -2''.63$

Of these results the last is most to be depended upon, as it is based upon the greatest number of observations.

2. The annular Eclipse of May 15th, 1836. Rümker's calculations (*Astr. Nachr.* xiv. p. 97) give the expressions for the moment of conjunction, but not the other results.

If we introduce, for the places whose observations embrace the commencement and the end of the eclipse, the condition that these two phenomena ought to give an equal value for the moment of the conjunction, we obtain the relations between the sum of the corrections of the semidiameters of the Sun and Moon $= x$, the difference of these two quantities $= y$, the correction of the latitude of the Moon $= \delta\beta$, and the correction of the parallax of the Moon $= \delta\pi$.

The results of Rümker's memoir give me, according to the method of least squares —

x	$= -1''.18 + 0.106\,\delta\pi = -0''.98$	Weight $=$	$72''.11$
y	$= -0''.69 + 0.026\,\delta\pi = -0''.64$	,,	95.06
$\delta\beta$	$= -0''.72 + 0.586\,\delta\pi = +0''.39$	,,	8.62

seeing that we have for the difference of the parallax according to Burckhardt and Adams

$$\delta\pi = +1''.90.$$

But as Rümker had already admitted in his calculations values

of $x = -1''$
,, $y = -2''$
,, $\delta\beta = -7''.63$

we have the following corrections of the data of the *Nautical Almanac* employed by Rümker:—

Total	x	$= -1''.98$
Total	y	$= -2.64$
Total	$\delta\beta$	$= -7.24$

of which the two first give—

Correction of Burckhardt's Semidiameter of the Moon	+ 0″·33
Reduction to Hansen's Tables	− 1·97
Correction of Hansen's Semidiameter	− 1·64

The observations of this eclipse do not agree with each other; I find the

Probable error of each observation	= 4ˢ·9
Therefore that of x	= 0″·58
,, y	= 0″·50
,, $\delta\beta$	= 1″·50
,,	$\frac{1}{2}(x-y) = \sqrt{(0''\cdot29)^2 + (0''\cdot25)^2} = 0''\cdot38$

D. Measurements with a heliometer.

1 and 2. Observations by Bessel of September 2, 1830, and of December 26, 1833, dates of a total Eclipse of the Moon. Each of Bessel's results is the mean of six diameters, taken at intervals of 30° of the angle of position. He finds,

	2 Sept. 1830.	26 Dec. 1833.
Correction of Burckhardt's Semidiameter ..	0″·00	+ 0″·73

But we have

Reduction to Hansen's Tables	− 1″·42	− 1″·78
Reduction to Hansen's Semidiameter ..	− 1″·42	− 1″·05

As to the difference of these two results, Bessel himself attributed them to the inaccuracy of the tables of the parallax (*Ast. Nach.* xi. p. 410). But M. Hansen's more accurate tables only reduce them one-half.

3. Measurements of Wichmann of July 1, 1846 (*Ast. Nach.* xxix. p. 1). In this series of observations the measurements follow at intervals of 5° of the angle of position. The observations were corrected for the refraction, according to the new formulæ. The phase was taken account of.

The measurements indicate that the Moon is not a sphere. The differences between the measured diameter are as high as 2″·48, a quantity which we cannot attribute to faulty observations. The mean gave,

Correction of Burckhardt's Semidiameter ..	+ 1″·26
Reduction to Hansen's Tables	− 2·66
Correction of Hansen's Semidiameter	− 1·40

4. Measurements by M. Peters during the total Eclipse of the Moon, of the 6th January, 1852, which were made under such unfavourable circumstances that I thought it better to reject the results. The eclipse began at $19^h\ 22^m$ mean time of Königsberg, while the Moon was at a height of 12° above the horizon, 38 minutes before sunrise. Peters himself said of them, "Der Mond erschien um diese Zeit wegen seines niedrigen Standes hinter Dünsten des Horizonts und wegen der eintretenden Morgendämmerung so schlecht erleuchtet dass ich nur die Schwächste Vergrösserung anwenden könnte. Mit dieser sah ich indess das Nord-und Süd-rand deutlich" (*Ast. Nach.* xxxiv. p. 11). The result was,

Correction of Burckhardt's Semidiameter—

By 5 measurements in the angle of position 0° ..	$= -1''.1$
6 ,, ,, 90°	$= +0''.8$
Mean	$-0''.15$
Reduction to Hansen's Tables	$-2''.09$
Corrections of Hansen's Semidiameter	$-2''.24$

Summary.

Corrections of the semidiameter of the Moon deduced from M. Hansen's Tables:

A. By occultations of Stars:

1.	Occultation of the Pleiades, August 29, 1820 ..	$-0''.86$
2.	,, θ' Tauri, March 28, 1830 ..	$-0''.49$
3.	,, α Tauri, February 10, 1832 ..	$-1''.11$
4.	,, the Pleiades, August 10, 1841 ..	$-1''.11$
	Mean	$-0''.89$

B. By total eclipses of the Sun:

1.	Eclipse of July 7, 1842	Olufsen	$-3''.50$	$-2''.80$
		Carlini	$-2''.09$	
2.	,, July 28, 1851			$-2''.30$
	Mean			$-2''.55$

C. By annular eclipses of the Sun:

1.	Eclipse of September 7, 1820	− 2″·63
2.	,, May 15, 1836	− 1·64
	Mean	− 2·13

D. By measurements with a heliometer:

1.	Measurements by Bessel, September 2, 1830 ..	− 1″·42
2.	,, ,, December 26, 1833 ..	− 1·05
3.	,, Wichmann, July 8, 1846 ..	− 1·40
	Mean	− 1·29

As to these results, it is evident that the total eclipses of the Sun point to a diameter smaller than that which the other phenomena explain. In fact, the observations here give us the disappearance and reappearance of the light of the Sun in the gaps presented by the indentations of the limb of the Moon. In the annular eclipses, on the contrary, we should find the diameter at its maximum, if the observers noted the moment when the limb of the Sun showed without any interception by the mountains of the Moon. But there are other observers who note the first and the last instant of the appearance and of the disappearance of the series of luminous points, and it is clear that their observations will give a minimum diameter. It is to this difference of conception that Nicolai already attributed the sensible difference between the observations of the same eclipse and at the same place, but by different persons. It therefore appears to me better to separate in the final result the date furnished by eclipses, from those based upon occultations and direct measurements.

I obtain:

Correction of the Semidiameter in M. Hansen's Lunar Tables—

For the calculations of occultations	− 1″·09
,, ,, eclipses	− 2·34

Without doubt there are many memoirs on the determination of longitude by means of the occultations of stars which would furnish us with ulterior data for the subject on which we are engaged. It is true that there are a great number of calculations which do not give the necessary differential equations. Wurm and Triesnecker have probably not even calculated them. But there is in the *Ast. Nach.* a very great number of calculations of longitudes. The index of the volumes i. to xx. has 14 pages, each of 2 columns, of them; that of vols. xxi. to xl. 3 pages. Besides there are 3600 observations of occultations which we find in the same journal, vol. i. to xlviii.

Lastly, we must not forget the millions of analogous observations communicated in the *Astronomisches Jahrbuch* (1776–1830), the *Allgemeine Geographische Ephemeriden* (1798–1800), the *Monatliche Correspondenz* (1800–1813), the *Zeitschrift für Astronomie* (1816–1818), the *Correspondance Astronomique* (1818–1826),—materials sufficient for an extended research, which my occupations do not permit me even to attempt. I shall content myself with having obtained a result which is sufficient for practical purposes. For we must not forget that the Moon is not a spherical body, and that the most perfect acquaintance with its mean diameter will not enable us to avoid small differences in the result of occultations.

For practice, then, I prefer to admit the following data:—

Relation of the diameter of the Moon to that of the Earth's equator.

According to Hansen	.. .. ..	0·272957
Correction corresponding to	$\Delta r = -1''{\cdot}09$ ..	− 318
	$\Delta r = -2{\cdot}34$ ..	− 684
Corrected relation ..	.. .. ..	0·27264
	.. .. ..	0·27227

Values of which that adopted by Burckhardt, 0·2725, is about the mean.

Finally, as the mean equatoreal horizontal parallax is 56′ 59″·57 according to M. Hansen, we have —

Semidiameter of the Moon:

	′	″
According to occultations and direct measurements by the heliometer	15	32·27
According to eclipses of the Sun	15	31·02

Batavia, April 20, 1859.

Results of some Observations on the Bright Granules of the Solar Surface, with Remarks on the Nature of these Bodies. By William Huggins, Esq., F.R.S.

I employ the word *granules* in preference to the other names* which have been proposed for the bright particles of

* Stone calls them *rice-grains;* Nasmyth, *willow leaves;* Dawes, *granulations* and *minute fragments of porcelain;* Chacornac, *crystals;* Brodie, *shingle-beach.* Sir W. Herschel's *corrugations* and *bright nodules* may refer to the groups of granules and possibly sometimes to single granules.

the solar surface, because, as Mr. Dawes, who suggested this term, well observes "the appellation *granulations* or *granules* assumes nothing either as to exact form or precise character."

In this paper I confine my remarks to the bright granules as they appear on those parts of the Sun which are free from the disturbing forces or currents which are active in the areas of the spots. Under the influence of the forces by which the spots are produced the bright granules assume different, and with respect to their appearance on the general surface of the Sun, irregular and unusual forms. In these regions of disturbance the granules often appear to coalesce, sometimes to be drawn out into very elongated forms, and occasionally to be wholly dissipated within the umbra of a spot. On those parts of the Sun however where the spots are absent these granules appear to preserve, within not very wide limits, considerable general definiteness of form, of size, and of mode of grouping. It is to these normal characters only of the bright granules that the observations of the present paper refer.

In a note I give references to the more important observations of others of these interesting bodies.*

Distribution.—With the exception, mentioned already, of the areas containing spots, the bright granules are to be seen over the whole surface of the Sun. Occasionally, granules preserving their normal characters may be detected in the penumbræ and the umbræ of spots. On one occasion, May 4, 1866, I resolved a large facula near the edge of the Sun's disk into an aggregation of similar particles. Without the border of the facula were two or three isolated granules of equal brightness with those composing the facula. These granules appeared as if they had become detached from the group forming the facula.

Form.—When the granules are observed with powers of about 100 diameters, no comparison which has been made appears to me so appropriate as that to "rice-grains," suggested by Mr. Stone. If, however, higher powers are employed, this apparent regularity of figure and of size of the granules disappears to a great extent. Many of them are then seen to be nearly round, and not of the elongated form of rice-grains. Besides the oval and nearly round granules, may be observed irregular-shaped masses of almost every form. An important character common to all these bodies, whatever their

* Dawes, *Monthly Notices*, vol. xxiv. p. 140 and p. 161; Nasmyth, ibid. vol. xxiv. p. 66; Stone, ibid. vol. xxiv. p. 124; Lockyer, ibid. vol. xxv. p. 237; Brodie, ibid. vol. xxv. 233; Fletcher, ibid. vol. xxv. p. 231; Huggins, vol. xxiv. p. 141; De La Rue, *Memoirs Royal Ast. Soc.* vol. xxxiii.; Chacornac, *Reader*, Jan. 7, 1865. See also for the interpretation of these bodies, Faye, *Comptes Rendus*, tom. lx. p. 138 and p. 468; Brayley, Knight's *Companion to the Almanac*, 1864, 1865, 1866, and *Ast. Register*, vol. ii. p. 75; De La Rue, Stewart, and Loewy, *Researches on Solar Physics*, First Series; Sir J. Herschel, *Monthly Notices*, vol. xxv. p. 152.

form, is the irregular broken outline by which they are bounded. If, however, these smaller irregularities of figure be disregarded the granules may be described generally as possessing a more or less oval form. The granules appear to me not to be flat disks, but bodies of considerable thickness.

In the interpretation of these bodies it must not be forgotten that minute irregularities which appear almost insignificant in our telescopes would not be little to an observer on the Sun. If the granules could be viewed from a short distance they would appear probably as wildly rugged in irregularity of outline as are the clouds of our sky.

Size.—On April 26, the Sun's image was allowed to pass before the wires of the micrometer placed at a small distance apart. The interval separating the wires appeared to include sometimes two and sometimes three of the granules which were in contact with each other. I found the value of the interval between the wires to be 2″·59. The average size therefore of those bodies may be taken roughly at 1″ in diameter, and the average longer diameter of the more oval particles at about 1″·5. This estimation agrees closely with the size assigned to them by Mr. Stone.

On some small areas of the solar surface the groups appeared to be composed of granules of nearly the same size, whilst on neighbouring areas a considerable difference in size existed between the adjacent granules. Occasionally a much larger granule was seen which might measure from 2″ to 3″ in diameter. Many of the granules were smaller than 1″ in diameter.

Relative position.—On many parts of the Sun the granules, though they lie near together, are detached bodies separated from each other by small intervals. These groups of isolated granules are mingled with tesselated surfaces of bright matter formed of close aggregations of granules. The forms assumed by the groups of closely united granules are very various. Often they appear as nearly round or oval cloud-like masses, and when in this form have been probably mistaken by some observers for single granules. Sometimes these groups are long, irregularly formed bands, suggesting to an observer the rugged, broken sides of a range of mountains. On April 26, nearly in the centre of the Sun's disk, I observed a long oval border of tesselated bright matter, enclosing an area over which the granules were sparsely distributed. Some of the more characteristic of the modes of grouping of the bright granules, which I have observed on different occasions, are given in a diagram which accompanies this paper.

It is in connexion with these groups that the coarse mottling of the solar surface originates, for the difference in brightness of adjoining areas is produced *mainly* by the greater or less degree of closeness of aggregation of the bright granules. A second cause which contributes to the formation of the mottled appearance of the Sun's disk is to be found in the

different degrees of brightness of the material which fills the intervals between the groups of granules, and between single granules.

In addition to these phenomena, a careful observer will notice appearances which suggest considerable inequalities of *level* in the bright surface of the Sun. The whole photosphere appears corrugated into irregular ridges and vales. Over this uneven surface, not unlike that of a stormy sea, the groups

Diagram of the Distribution of the Bright Granules on the parts of the Sun which are free from Spots.

of granules which have been described are irregularly distributed.

Superposition?—I have not been able to satisfy myself whether the material of the photosphere immediately beneath the bright granules consists of an aggregation of separate particles. The appearances presented suggest that this matter is lower (nearer the Sun's centre), and that generally the granules are

bodies of considerable thickness, elevated above it, and surrounded by gaseous matter which is non-luminous in comparison with the extreme splendour of the granules. So superior in brightness are the granules, that the exterior layer, which is composed exclusively of them, must be regarded as the source of nearly the whole of the light, and probably also of the heat, which the Sun emits. Except in the penumbra of a spot, under the influence of unusual currents, I incline to the opinion that the bright granules are not superposed on each other as long as they remain recognisable as such.

The phenomena would be well represented if we might suppose that the granules are recently condensed incandescent clouds, that they slowly sink, merge into each other, become less and less luminous, and gradually dissipate into comparatively non-luminous gas. The dark pores would then be represented by the portions where complete vaporization had taken place.

Nature.—Mr. Dawes states that, after years of careful observation of these bright bodies, he considers them to be "merely different conditions of the surface of the comparatively large luminous clouds themselves—ridges, waves, hills, knolls, or whatever else they might be called—differing in form, brilliancy, and probably in elevation." I would venture to differ from this distinguished observer only so far as to suggest that the bright granules were originally *separate* clouds, though it may be that their under surfaces soon begin to unite with the less luminous stratum of clouds beneath them.

I cannot express the ideas suggested to myself by observations of the Sun more accurately than in the words of Sir John Herschel—"That it is hardly possible not to be impressed with the idea of a luminous medium intermixed but not confounded with a transparent and non-luminous atmosphere."

The extreme mobility and other phenomena of the bright matter suggest that it is present in the form of *cloud.*

The knowledge which the spectroscope affords of the chemical and physical constitution of solar matter, together with the phenomena of terrestrial flame, would suggest that in the greatly different powers of radiation possessed by matter in the solid, liquid, and gaseous state, combined with the processes of condensation and revaporization, a feasible explanation might be found of solar phenomena. However, the law of exchanges, for which we are greatly indebted to the original and important researches of Balfour Stewart, shows that in the case of the Sun, if we suppose it to be, at the least, equally hot throughout its mass, the gas near the surface would not appear dark, as the umbra of a spot or a dark pore does, because its own feeble power of emission would be supplemented by its power in the inverse ratio of transmitting the radiation from the gas, or from the photosphere behind it.

At present, therefore, we do not appear to be able to explain satisfactorily the small brilliancy of the material between the bright particles, and of the darkness of the pores, which, like the umbra of a spot, appears to be at a lower elevation than the bright matter. It has been recently suggested that the dark umbra of a spot originates in a lower temperature produced by a down rush of cooler gas from above. It is of importance to know whether this hypothesis can be made to afford a probable explanation of the numerous dark pores and the small brilliancy of the material between the groups of bright granules.

If the granules are incandescent clouds, their general oval form may possibly be due to the influence of currents. We have to seek, besides the conditions connected it may be with the high temperature of the Sun, or with some peculiarity when in the gaseous state of the substances present in the photosphere, in consequence of which there exists a general approximate uniformity of size, of form, and of mode of grouping of these incandescent bodies.

Description of an Equatoreal Clock. By Lord Oxmantown.

The following description of an apparatus for giving an equable motion to a heavy Equatoreal of 18 inches aperture, may, perhaps, be interesting to some of the Fellows.

It was constructed last autumn, and its merit, if it has any, is that it can be very easily made. No nice workmanship is required; a joiner and plumber can execute the work with sufficient accuracy.

The motive power is a piece of wood closely fitting a wooden box containing water, on the surface of which this piece of wood floats. This float is covered with canvass saturated with pitch, to prevent it from imbibing water, and so expanding and sticking fast in the box.

It has also pieces of sheet-brass fastened on the edges over the pitched canvass, to prevent the pitch adhering to the sheet-lead with which the box is lined. A tube from the bottom of the box, terminating in a piece of flexible pipe, the extremity of which is kept at a uniform depth below the surface of the water in the box by being suspended from the float, allows the water to escape at a constant rate which is regulated by a valve in this pipe. (*h h*) is the box; (*i i*) the float, which is attached to a brass tube (*d d*), which acts as a guide by passing through a hole in the cross-piece of wood (*f f*), supported by two upright pieces (*g g*); (*n n*) is the India-rubber tube, through which the water escapes; (*o*) the valve to regulate the flow; (*m*) is a cock, which is closed when the clock movement is not required; (*q q*) a second box to receive the water as it flows out of the first. When we wish to employ the clock, we, first, pump up water from (*q*) to (*h*)

through (p) and (l); secondly, we get the object to be observed into the centre of the field; thirdly, open the cock (m) and clamp the sector (t) to the polar axis (z) by means of the screw (u); and if the object is not now sufficiently in the centre of the field, it may be brought there accurately by turning the nut (c), which draws the sector (t) further from or nearer to the float, by bending the spring (w).

The accuracy of this clock depends, to a great extent, upon the care which is taken to make the sides of the box parallel and flat, so that the horizontal section may be the same at all levels, and to fit the float to it carefully, so that the interval between the float and the sides of the box may be as small as is consistent with absence of friction. Let A be equal to the area in square inches of horizontal surface of float, and $\frac{A}{n}$ area of interval between the float and sides of the box; then, if the float be displaced $\frac{1}{m}$th of an inch from the position of equilibrium, the surface of the water round the float will be displaced through $\frac{n}{m}$ inches; therefore, if w = weight in lbs. of 1 cubic inch of water, the force tending to bring the float back to its former position = $w \,.\, A \left(\frac{n}{m} + \frac{1}{m}\right)$; whereas, if the float had rested on a surface of water of infinite extent, the corresponding force would have been $w \,.\, A \times \frac{1}{m}$ $\therefore$ the ratio of the forces in the two cases = $n + 1 : 1$. In the case of the instrument to which this apparatus has been applied, A = 1296, and if, for example, the interval between the float and sides of the box = $\frac{1}{16}$ of an inch, and if the variation of friction of the polar axis be taken at 3 lbs. (the average friction being about 7 lbs.), the displacement of the float, which would have been equal to ·064 of an inch on a surface of water of infinite extent, will become equal to

$$\cdot 064 \times \frac{1}{1 + \frac{1}{4 \times 36 \times \frac{1}{16}}} = \frac{\cdot 064}{145} = \cdot 00044 \text{ inches.}$$

As the float is attached to a wire rope the other extremity of which wraps round the sector (t) of 28 inches radius, this deviation corresponds to 7′ 51″ of space at the equator in the first case, but only 3″·2 in the second case.

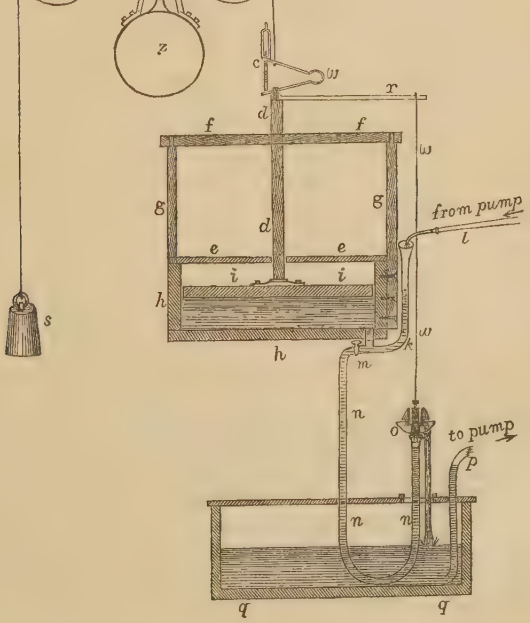
Assuming that the flow of the water from the box will be uniform if the head be uniform, the only remaining cause of irregularity lies in this, that the overflow is kept at a constant depth below any fixed point in the float; that point, through variation of the resistance, not being at an absolutely invariable distance below the surface of the water in the box; consequently, the head will vary if the immersion of the float varies.

We have seen above that, with a variation of friction of 3 lbs., the variation of immersion = ·064 of an inch; therefore (47 inches being the distance of overflow below the surface of water) the ratio of maximum velocity to mean $= \sqrt{\frac{47 \cdot 064}{47}} = \frac{1 \cdot 00067}{1}$, which corresponds to an angular deviation in right ascension of 36″ of space at the equator per hour.

In practice, however, when the instrument is accurately counterpoised and carefully oiled, the variation of the force required to turn it round in right ascension is probably very much less than this; and if between one night and the next the friction increases through viscosity of the oil, the rate is easily corrected by means of the valve (*o*).

This clock has been used for micrometrical observations, and answers perfectly; but how far for spectroscope purposes it will bear comparison with other contrivances, has not yet been ascertained.

The accompanying figure has been drawn on a scale of $\frac{1}{3}$ inch to 1 foot, with the exception of the sector (*t*) and the weight (*s*), which keeps the rope (*x*) extended; these two parts having been drawn on a reduced scale, and in a different position to save space.



On the Satellite of Sirius. By M. Otto Struve.

Two years ago I had the honour to communicate to the Royal Astronomical Society the first results of my micrometrical measures of the satellite of *Sirius*, discovered by Mr. Alvan Clark. Though at that time the comparison of the observations made in 1864 with those of the preceding year strongly favoured the supposition that the small star did not participate in the large proper motion of *Sirius*, and, consequently, was not physically connected with it, I indicated the reasons which induced me to discuss these appearances, and suspended therefore the judgment about that conviction at least until the next year, when the differences between the orbital motion and the relative motion in the supposition of a merely optical juxtaposition would have increased to nearly the double amount. At present, after having two years' observations more, we are enabled to submit the question to a stronger examination.

The following list contains all measures that I have proceeded until now to make on the satellite, after having excluded those days on which the state of the atmosphere was indicated as so unfavourable that it did not admit an accurate measure of the distance:—

Date.	Sid. Time.	Power.	Distances.	Position Angle.	
				Observed.	Corr.
	h m		″	°	°
1863·21	7 30	309	10·30	79·8	82·0
63·21	7 11	309	9·99	81·2	83·0
64·18	6 37	309	11·22	75·2	76·7
64·24	7 11	309	10·80	75·6	77·4
64·24	7 33	412	10·73	74·3	75·3
65·17	5 45	(300)	10·39	76·0	77·3
65·24	8 20	(300)	10·80	73·8	77·0
66·19	5 55	309	11·20	73·3	74·7
66·20	6 56	309	10·90	74·4	76·1
66·23	7 4	309	10·70	73·5	74·7

The column headed "Corr." gives the position angles corrected for the systematical errors, as deduced from my observations of artificial double-stars. It might be remarked on this occasion, that by later researches these corrections are proved to have full strength, as well for natural double-stars in all zenith distances, as for artificial ones near the horizon. Concerning my observations, I have further to say that those of the spring, 1865, have been made at Rome, where Father Secchi had kindly offered me for that purpose the use of the excellent 9-inch refractor of the Observatory of the Collegio

Romano. For the correction of these Roman position angles, I have employed estimated values of the magnifying powers. A small error in this supposition would be of no consequence.

The arithmetical means of each year's observations are—

1863·21	$\rho = 10''{\cdot}15$	$P = 82^\circ{\cdot}50$	2 days.
64·22	10·92	76·50	3 ,,
65·20	10·60	77·15	2 ,,
66·21	10·93	75·15	3 ,,

Adopting now the same weight for these four results (a supposition that appears necessary if we admit for different years different constant errors of estimation with regard to the centre of the bright star), the method of least squares conducts us to the following formulæ for a uniform rectilinear relative motion in the course of three years:—

$$p = + (10''{\cdot}186 \mp 0''{\cdot}193) + (0''{\cdot}121 \mp 0''{\cdot}046)\ (t - 1863{\cdot}0)$$
$$q = + (\ 1{\cdot}523 \mp 0''{\cdot}193) + (0{\cdot}429 \mp 0''{\cdot}046)\ (t - 1863{\cdot}0)$$

p and q being the rectilinear co-ordinates of the satellite from the principal star. From these formulæ we deduced the observed change for three years; in distance $= + 0''{\cdot}63$; in position angle $= - 6^\circ{\cdot}38$.

If the small star formed only with the large one an optical double star, the mean proper motion of *Sirius* would have changed in three years the relative distance by $+ 2''{\cdot}56$, and the angle of position by $-15^\circ{\cdot}02$,—values that might be increased to $+ 2''{\cdot}81$ and $-16^\circ{\cdot}17$, if we introduce the ascertained inequalities of that proper motion. Evidently our observations do not agree with similar changes; therefore this hypothesis must henceforth be given up.

Moreover, the excellent investigations of Dr. Auwers (*Ast. Nach.*, No. 1506), on the orbital motion of *Sirius*, demands for the disturbing body in the indicated interval an increase of the distance by $0''{\cdot}55$, and a diminution of the position angle by $5^\circ{\cdot}31$; values agreeing so nearly with those deduced from our micrometrical measures, that we scarcely can doubt that the satellite discovered by Mr. Alvan Clark is really the disturbing body.

The distance itself cannot form in this case a criterion for the identity, for the irregularities of the proper motion could only furnish the distances of the common centre of gravity. The distances of the disturbing body, as given by Dr. Auwers, must therefore have been derived from a supposition of the relative masses of the two bodies. Putting $d = 3{\cdot}05\ \Delta$ (Δ being the distance of the centre of gravity), Dr. Auwers, it seems, has adapted the supposition on the masses to the distances of the satellite, as observed before his Note of 1864 was

drawn up. Our measures agree so nearly with the supposition that the mentioned relation would only be changed into $d = 3{\cdot}087\ \Delta$. Of course, even a considerably more important change in this supposed relation, would affect the change of the distances, during the short interval of our observations, only by very small quantities.

With regard to the position angles, on the contrary, not only their changes, but also their values themselves, as measured on the satellite, must agree with those deduced for the centre of gravity of the system, if really that satellite is the body that has produced the irregularities of the proper motion. Here we have the following comparison for our yearly measures:—

1863·21 Str.—Auw.	$= -0^{\circ}{\cdot}96$
64·22	$-5{\cdot}11$
65·20	$-2{\cdot}73$
66·21	$-2{\cdot}93$

This near agreement is certainly one of the strongest indications for the supposed identity. In the mean the difference is $-2^{\circ}{\cdot}94$, which at the mean distance $10''{\cdot}64$ corresponds to $-0''{\cdot}55$ in space. I do not think this difference can be attributed to the accidental errors of my observations, and even the supposition of constant errors of estimation with regard to the centre of the bright star as depending from the sensibility of the eye for the colours of the atmospheric spectrum of *Sirius*, is at least rendered much less probable by the circumstance than my Roman observations, where that star in its higher elevation did not offer any trace of spectrum, very nearly agree in amount and sign with the mean of the other three determinations. Therefore it seems that in this respect Dr. Auwers' theory is still susceptible of a small correction. That such a correction might be desirable will certainly not appear surprising, if we consider the unequal qualities of the numerous observations from which he has derived the disturbances of the proper motion.

Admitting then that the observed satellite is identical with Bessel's obscure body, the above given relation $d = 3{\cdot}087\ \Delta$, indicates, that its mass must be estimated approximately half that of *Sirius* itself. If both bodies had the same physical constitution, this relation of the masses would assign to the globe of the satellite a diameter only 1·26 times smaller than that of the principal body, and therefore, considering the extraordinary brightness of the large star, we would be induced to place also the satellite in the first class of magnitude. With this conclusion the observed brightness of the companion forms a manifest contradiction. It is commonly said to be of the 9th or 10th magnitude; and only in the Spring of 1864 I have noted it once as of the 8th magnitude, probably on account of

an extraordinary favourable state of the atmosphere. Hence it follows that, to maintain the identity, we must admit that both bodies are of a very different physical constitution. That the light of the satellite is gradually increasing, as I was inclined to suppose from the comparison of my observations of 1864, with those of 1863, has not been confirmed by later observations; but in our latitude the estimation of the brightness depends too much from the condition of the atmosphere to admit of an accurate judgment in this respect.

Pulkova, April 15, 1866.

Supposed Observation of Biela's Comet.

By James Buckingham, Esq.

At our evening Meeting in March, an inquiry was made "if Biela's Comet had been seen;" having mentioned an observation I obtained on November 9 last of two bodies which were supposed by me to be those of that Comet, our President requested that "details should be furnished to the Society." But business affairs caused me to leave London the following day, and I have only returned a few days since, and now beg to communicate the observation, viz.—

Comets + Star.

Difference of R.A.

Nov. 9, 1865, at 9 h 32 m G.M.T. A + 28 s and was some minutes N. of B.

B + 37·75 same decl. as ✳ comparison.

	h	m	s		°	′	
✳ *a* is 12th or 14th mag.	23	18	35	R.A.	77	25·1	N.P.D.
Deduced place of A	23	19	3				
B	23	19	12·75		77	25	

Owing to clouds, a second position could not be obtained.

It may be necessary to explain why no better observations could be secured, having usually at hand well-appointed means. But having been many weeks away in Scotland, returning only in the beginning of November, I was favoured with Mr. Hind's Ephemeris of Biela, and sought several nights with my 9-in. Equatoreal (by Wray, and which is a very fine one) unsuccessfully, I therefore put up my 20-in. object-glass (the instrument having been out of use for several months, for some alterations of the mounting, and was only approximately in the meridian), after searching near the predicted places, in sweeping found two round vapory bodies near each other; after watching several minutes motion was detected (from n.f. to s.p.) of the small one, which appeared most condensed, but without any

sign of nucleus, but yet with nearly defined outline. I had no micrometer, but only a pair of thick cross-wires in the eye-piece (negative), which are seen clearly against the sky; and observing a small star "*a*," apparently on the same parallel as the brightest object; got some transits and approximate positions without being able to determine their accurate ones, but the star *a* was nearly north of 70 *Pegasi*; therefore went to my 9-in. Equatoreal, but could not see any trace of the new objects. On returning again to the large (20″) instrument, again saw both objects, and the one B I thought had moved yet further and past a very minute star, in the same direction as before; but clouds came on, and no opportunity occurred, as on 12th November I left London, and did not return till the 23rd, when I again searched, but could not find either of the new objects, but found the star of comparison and determined its place, and having made a rough ephemeris, by using the observed differences on November 9 and Mr. Hind's calculated places, swept for several weeks at every opportunity. I need perhaps scarcely say "my conviction is, the bodies observed were comets;" and from the circumstance of being near each other, although closer than calculated, it is presumable, as so near its orbit, they were Biela's.

A gentleman at the Meeting in March remarked he "had a doubt if motion could be detected in half an hour." There could not be the slightest doubt of the fact, and taking Mr. Hind's ephemeris as very good data, it appears the absolute arc in that time would be about 26 or 27 seconds; a distance impossible under like circumstances to be overlooked.

I must apologise for the length of this letter, but must mention I had the honour of an interview with Mr. Hind on the morning of November 10, who patiently heard my description, was much interested, but had a very strong impression "the bodies observed could not be Biela, in consequence of the places not agreeing with the calculated ones, and so close together, but that they were nebulæ;" and therefore no communication was made to the Society, as I hoped to obtain future positions, but was prevented, as described, by leaving town.

Westmorland House, Walworth Common,
London, S., May 4, 1866.

Note on Kirch's Variable in Collo Cygni.

By E. J. Stone, Esq.

In 1686 Geo. Kirch discovered that a star in *Collo Cygni* was subject to considerable variations of magnitude. This star was considered by Kirch to be identical with the 5th-magnitude star χ *Cygni* of Bayer. In Flamsteed's *Historia Cælestis Britannica* will be found observations of a 5th-mag-

nitude star in *Cygnus*, which is there called χ *Cygni*. This star is not the variable. Flamsteed's star will be found in Bessel's *Fundamenta*, No. 2517, and upon it Bessel remarks, "Flamsteedius stellam per χ designat, sed stella a Bayero ita dicta alia est neque reperitur in Catalogo." The neighbouring 5th-mag. star is contained in *Piazzi*. Piazzi has followed Flamsteed's nomenclature. The star in Piazzi xix. 295, *Cygni Variabilis*, 6.7 mag., would appear to be the star *e* of Kirch's map. It certainly is not Kirch's variable. In the notes to Baily's *Flamsteed* will be found the following, "17 *Cygni* χ. M. Bessel does not think that this is the star designated by the letter χ in Bayer's map. But there is no other star of that magnitude (5th) observed by Flamsteed or Piazzi that comes nearer to the position there indicated. I apprehend that the error is in Bayer, as there is no star in the Catalogue either of Ptolemy or Tycho that will accord with the position he has given." Baily has been followed in these remarks by Admiral Smyth, for in *Celestial Cycle*, page 460, vol. ii. we find, "This extraordinary star has been mistaken for χ, a 5th-magnitude star, which precedes." A careful comparison of Bayer's map with that given by Kirch, *Philosophical Transactions*, No. 343, 1715, will, I think, most clearly show that the variable is really the χ *Cygni* of Bayer. It would appear that the variable must have been near its maximum at the time Bayer was engaged on this part of the heavens, and that the variable was then brighter than the neighbouring 5th-magnitude star of Flamsteed, to which no letter has been appropriated by Bayer. This circumstance is to be regretted. Flamsteed's star is contained in Piazzi and many of our modern standard Catalogues, and is in them called χ *Cyyni*. From our having two stars both called χ *Cygni* many mistakes have arisen. Mr. Pogson, in his lists of Variables, *Radcliffe Observations*, 1854, and *Monthly Notices*, 1856, Dec. 12, has given the R.A. and N.P.D. of Flamsteed's χ *Cygni*, instead of those of the variable. In subsequent lists Mr. Pogson has corrected this error. But the same mistake will be found in all the editions of Sir J. Herschel's *Outlines*, Chambers' *Hand-book*, and probably in other works. I am of opinion that some steps should be taken to at least palliate the confusion at present existing from our having two stars called χ *Cygni*. No method entirely without objection can probably be proposed. I think, however, that the least objectionable method would be to call the star contained in Flamsteed, Bradley, Piazzi, and our chief modern Catalogues, χ_1 *Cygni*, and then to call the variable χ_2 *Cygni*. We should thus have for

1866, Jan. 1.

		R.A.	N.P.D.
		h m s	° ′ ″
χ_1 Cygni	5 mag.	19 41 20·38	56 34 56·67
χ_2 Cygni	variable	19 45 25 ±	57 26 ±

Spectrum of α Orionis. By the Rev. Father Secchi.

The rather too short letter which I wrote, accompanying the drawing of the spectrum of *α Orionis*, has given occasion to some remarks and inquiries of Mr. Huggins, which require some explanation on my side.

First, I must say that there is no slit in my apparatus, as for the stars it is quite useless and troublesome: the position of the lines is determined and measured as with the double stars. The *original* figure which I sent is made on the scale of one centimeter to one revolution of the micrometrical screw of the eye-piece. It would have been very easy to form a table of such values, but every one can make it, applying a metrical rule to the original figure. All the large lines have been put down by micrometrical measures: the smaller have been delineated by separating the wires only for one revolution, and putting them at the due place. The scale being large, no chance of error exists.

What Mr. Huggins says, that the colours in my figure are not right, is true; but this is an effect of artificial light which changes the tones of the colours; they ought to be omitted altogether. To fix the lines D and C, I have made use of a spectroscope with slit, and lights of sodium and magnesium; therefore they are correct.

The chief point of difference in my spectrum and that of Mr. Huggins lies in the band indicated with the letter ε in my figure, which is placed by me nearer to ζ than to δ, while in Mr. Huggins' it is quite the opposite (ζ is 1151 of H). This difference is so great that no micrometrical measure could be necessary to acknowledge it.

Not trusting to myself only, I have asked several other persons to try the position, and they agreed with me. I have therefore nothing to correct in my drawing, which was made before seeing those of Mr. Huggins', and carefully verified several times afterwards.

I do not say that there is a mistake of Mr. Huggins: on the contrary, I beg him to verify the measures. As he says that the nebulous band ending with the strong line 1069·5, then he gives perfect reason to me; since this is exactly the band which is controverted, and which I have not seen; but, instead of it, I have seen one in the place nearly 1110 of his scale.

We are, therefore, agreeing perfectly in the deficiency of the band ending 1069·5, but he must inquire if this is not appeared in the place 1110 of his scale.

I did not intend to intrude into the discovery of Mr. Huggins in the lines of *Sirius* and *Rigel*, but only to confirm his discovery. My observation is rather more detailed, since, for the intervals of the lines of *Sirius* and *Rigel*, I have been able to fix the proportion of $6\frac{1}{2}$ to $4\frac{1}{2}$. As to the similarity of these

spectra with those of Plücker, I have nothing to retract; the number of lines between the red and blue of the ring in *Sirius* and in *Sulphur* is most nearly the same.

Rome, April 16, 1866.

On a New Star. By W. Huggins, Esq.

On May 16 I received a note from Mr. John Birmingham, of Tuam, in which he informs me that, on the night of May 12th, he saw a new star near ε *Coronæ* B. He describes the star as "very brilliant, of about the 2d mag." The same day a letter arrived from Mr. Baxendell; he observed the star on the 15th, and found it to be fully equal in brightness to *β Serpentis* or *γ Herculis*.

On the night of the 16th, I and Dr. W. A. Miller examined the spectrum of this strange star. The results of our observation were communicated on the 17th to the Royal Society.

On the 16th, the star was brighter than ε *Coronæ* B, fully ·5 mag., perhaps ·75 mag. brighter than ε. On that evening a very faint nebulosity was seen extending some little distance round the star, and gradually fading away at its outer boundary. A comparative examination of neighbouring stars showed that this appearance of nebulosity was due to the star itself. On the evenings of the 17th, 18th, 19th, and 21st, no certain indications of a nebulous light about the star could be detected.

The spectrum of this star is very remarkable, and leads to unexpected conclusions as to its physical condition.

The light of the star is compound, and has emanated from two different sources. Each light forms its own spectrum. The principal spectrum is analogous to that of the Sun. The portion of the star's light represented by this spectrum was emitted by an incandescent solid or liquid photosphere, and suffered partial absorption by passing through an atmosphere of vapours existing at a temperature lower than that of the photosphere.

This absorption-spectrum contains two strong lines, a little more refrangible than C of the solar spectrum; a shaded group of lines extending nearly to D; a faint line coincident with D; numerous fine lines up to about the position of *b* of the solar spectrum, where a series of groups of strong lines commences and extends as far as the spectrum can be traced.

The second spectrum, which in the instrument appears to be superposed upon the one already described, consists of five *bright lines*. This order of spectrum shows that the light by which it was formed was emitted by matter in the state of *gas*.

One of these bright lines is in the red at the position of

Fraunhofer's C. The brightest of the lines coincides with F; a little beyond this is a fainter line; near this line a fourth occurs, which is either double or undefined at the edges. In the more refrangible part of the spectrum, probably not far from G, a fifth bright line was seen by glimpses.

On the 17th I observed this spectrum of the star simultaneously with the bright lines of hydrogen produced by the induction spark. The brightest line coincided with the centre of the expanded green line of hydrogen. Apparently the red line also coincided with that of hydrogen, but the faintness of the star spectrum did not permit the coincidence to be observed with certainty.

These bright lines are all much more brilliant than the corresponding refrangibilities of the continuous spectrum over which they fall. We must, therefore, conclude that the temperature of the gas by which the light, consisting of these five refrangibilities only, was emitted, must be much higher than that of the stellar photosphere from which the principal part of the star's light has emanated.

On the 17th, 19th, and 21st, the spectrum of the star was again examined. No important changes had occurred. In the faint spectrum of the fading star, on the 21st, both spectra could be seen. Some additional groups of absorption-lines are probably present in the continuous spectrum, but the gaseous spectrum is not changed otherwise than in the diminution of its brilliancy. The sudden blazing forth of this star, and then the rapid fading away of its light, suggest the rather bold speculation that, in consequence of some great internal convulsion, a large volume of hydrogen and other gases were evolved from it. The hydrogen, by its combination with some other element (the other bright lines do not coincide with those of oxygen), giving out the light represented by the bright lines, and at the same time heating to the point of vivid incandescence the solid matter of the photosphere.

The grouping of the dark lines of the absorption-spectrum is similar to that which characterises the spectra of *α Orionis* and *β Pegasi*, stars in the atmosphere of which *no hydrogen exists*.

We have found that several of the more remarkable of the variable stars which have an orange tint have spectra similar to those of *α Orionis*. It is worthy of notice that all the white and bluish-white stars have spectra in which, on the other hand, the dark lines due to absorption by hydrogen are very strong, whilst all the other lines are thin and faint. These and other observations we have made suggest that hydrogen probably plays an important part in the changes and physical differences of constitution of the stars.

The grouping of the lines of absorption of this new star shows that its colour would be orange, if it were not for the greenish-blue light of the bright lines, which more than

makes up for the refrangibilities which have been intercepted by absorbent vapours. Mr. Hind writes, "On the 19th, the star was perfectly white and stellar to my eye." Mr. Baxendell's remarkable powers of observation enabled him to divine some of the results of prismatic examination which at that time were not known to him. He writes, "On the 18th I several times received the impression of a bluish tinge as if the *yellow of the star were seen through an overlying film of a blue tint.*"

On the 17th, the star's exact position was obtained at Greenwich. The Astronomer Royal informs me that a meridian observation showed that this strange object "agrees precisely with Argelander No. 2765 of 'Bonner Sternverzeichniss,' declination + 26°, magnitude 9·5." Mr. Baxendell writes to me, "It is probable that this star will turn out to be a variable of long irregular period, and it may conveniently be at once designated T *Coronæ*." Mr. Baxendell gives, in his letter, the following table of the magnitudes of this wonderful star:—

"A reduction of all my observations gives the following results:—

1866		h	m			Mag.
May 15	at	12	0	G.M.T.	T Coronæ	= 3·6 or 3·7
16	,,	10	30	,,	,,	= 4·2
17	,,	11	0	,,	,,	= 4·9
18	,,	12	30	,,	,,	= 5·3
19	,,	12	15	,,	,,	= 5·7
20	,,	12	30	,,	,,	= 6·2."

A diagram of the spectrum, taken from the *Proceedings* of the Royal Society, will, by the permission of that Society, appear in the next number of the *Monthly Notices*.

Prof. Gould, in a letter dated Cambridge, U.S., April 20, 1866, addressed to the President, calls attention to a paper by Mr. Ferrel in the year 1853, in which it is shown that the action of the Moon on the tidal protuberances produces an appreciable retardation in the velocity of rotation of the Earth.

Mr. Ferrel's paper, published in the *Astronomical Journal*, vol. iii. pp. 138–141 (Dec. 8, 1853), is entitled "On the Effect of the Sun and Moon upon the Rotatory Motion of the Earth." After some preliminary remarks, the author proceeds as follows:—

"Assuming the Earth to be a perfect sphere, the effect of the Moon is to raise the waters of the ocean on the opposite sides, and to cause the mass to assume the form of a prolate spheroid.

If the Earth remained a perfect sphere, or if the major axis of this spheroid coincided with the line joining the centres of the Earth and Moon, the attraction of the Moon could not affect the Earth's rotatory motion. But in consequence of the resistance which the waters of the ocean suffer in their motions, the major axis of this spheroid forms, with the line joining the centres of the Earth and Moon, an angle of about 30°; and the well-known tangential force relative to the centre of gravity, arising from a difference between the attraction of the Moon upon the different particles of the Earth and the centre of gravity, and also from a difference in the direction of this attraction, is not exactly balanced in its action upon the different sides of the mass, in consequence of two opposite parts, in which this force acts in the same direction, being a little more prominent than the other two on account of the spheroidal form which the mass assumes. Hence there is a small residual force of the order a^2, which, acting continually in the same direction, has a tendency to produce a variation in the value of dA.* This force would act upon the waters of the ocean alone, producing a western current, and would not affect the nucleus at all if it did not offer resistance to their motions; but as soon as this resistance becomes equal to the force, the whole force is spent indirectly upon the nucleus; and as, by the principle of the preservation of areas, the action of the particles upon each other cannot change the value of A, the effect is the same as if it acted directly upon the nucleus."

He then proceeds to a mathematical investigation of the foregoing effect; this is presented in what may be thought a form of unnecessary complexity, in consequence of the assumption of a hypothetical law of density of the solid nucleus:† but the result obtained is

$$nt - u = \frac{39\,\mathrm{L} \cdot h \cdot \sin 2a}{\varrho^3 (36\,m + 16\,m')}\, t^2$$

where

$1 + h$ = Semi-axis major of the spheroid, the semi-axis minor being $= 1$.

m = Mean density of the Earth.

m' = Density of nucleus at the surface.

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* a, the order of the disturbing forces; A, the moment of rotation, or sum of the products of each particle into the area described by its radius vector.—Ed.

† The assumed law is—

$$\text{Density} = m' + c\,(1 - r^2),$$

r being the distance from the centre (the radius being unity) and c being so determined that the mean density shall be $= m$; or finally

$$\text{Density} = 3\,m - 2\,m' - 3\,(m - m')\,r^2.$$

L = Moon's attraction at distance unity.

ϱ = Distance of centres of the Earth and Moon.

a = Angle between line forming the centres of the Earth and Moon, and the major axis of the spheroid.

nt = Angular motion of Earth when not acted on by the disturbing force.

u = Actual angular motion.

For converting the foregoing formula into numbers the author writes as follows:—

"Now the attraction of the Earth at the distance of the equatoreal radius, is 32·148 feet; and putting the attraction of the Moon $\frac{1}{75}$ of that of the Earth, we shall have

$$L = \frac{32 \cdot 148}{75 \times 20922000} \qquad (17)$$

in times of the Earth's radius. Assuming that the tide caused by the action of the Moon in the open sea is two feet, and that it happens two hours after the Moon passes the meridian, we shall have $h = 2$, and the angle $a = 30$, and consequently $\sin 2a = \cdot 66602$. We will also put $\varrho = 60$, $m = 5\frac{1}{2}$, and $m' = 2\frac{3}{4}$, and $t = 100$ years $= 3155760000$ seconds. These values will give $nt - u = 50 \cdot 17$ miles for the amount the Earth would be retarded at the Equator in a century, upon the hypothesis that the whole nucleus is covered with water. But as the waters of the ocean do not completely assume the spheroidal form on account of the continents, which occupy about one-fourth of the surface, we must deduct one-fourth from the value of $nt - u$, and we shall then have for the effect of the Moon in one century

$$nt - u = 37 \cdot 44 \text{ miles} \qquad (18)$$

Now if in the equation (17), L be increased or diminished, h will be increased or diminished in the same ratio; hence the effect of any other body in changing the value of $nt - u$ will be as the square of its effect in causing tides. But this effect of the Sun is $\frac{3}{7}$ths of that of the Moon, and consequently its effect upon the value of $nt - u$ will be $\frac{9}{49}$ of that of the Moon. We shall therefore have for the effect of both Sun and Moon,

$$nt - u = 44 \cdot 45 \text{ miles} \qquad (19)$$

If the motion of the Earth were retarded by this amount it would cause an apparent acceleration of the heavenly bodies in their orbits, which in the case of the Moon would amount to $84''$ in a century. And as no such acceleration has been observed above what is accounted for otherwise, the rotatory motion of the Earth must be nearly uniform, and the above

effect of the Sun and Moon must be counteracted by the gradual contraction of the Earth through a loss of temperature."

And, in conclusion, the author calculates that, to counteract the foregoing acceleration of 84″ per century, there is required a contraction of about 1·017 feet in the Earth's radius during a century, upon the hypothesis of an equal contraction throughout the mass.

Leyton Astronomical Observations.

In revising the proof-sheets of the above volume, I find an error in the arrangement of Comet Observations has escaped my notice; at p. 76, the observations for 1864, Feb. 8, 9, 16, 18, and March 1, belong to Comet V. and not to Comet IV.

This is immediately evident by turning to the comparison-stars at p. 95. C. G. TALMAGE.

Mr. Barclay's Observatory, Leyton.

ERRATUM IN THE LAST NUMBER.

In the list of Fellows elected, *for* George Hurst, Esq., *read* George Hunt, Esq.

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